

Good morning. I would like to begin by summarizing our previous testimony. The debate over Dr. Mann's principal components methodology has been going on for nearly three years. When we got involved, there was no evidence that a single issue was resolved or even nearing resolution. Dr. Mann's RealClimate.org website said that all of the Mr. McIntyre and Dr. McKittrick claims had been "discredited". UCAR¹ had issued a news release saying that all their claims were "unfounded". Mr. McIntyre replied on the ClimateAudit.org website. The climate science community seemed unable to either refute McIntyre's claims or accept them. The situation was ripe for a third-party review of the types that we and Dr. North's NRC panel have done. Because of the very high visibility of the original study, we see no harm and much advantage of having two independent analyses of the situation, from quite different perspectives.

While the two studies overlap on the important topic of Mann's principal components methodology, the Dr. North's NRC panel considers topics that were outside the scope of our study, such as other temperature reconstructions. Where we have commonality, I believe our report and the NRC panel essentially agree. On the error in the use of principal components methodology, the NRC panel reported, "...under some conditions, the leading principal component can exhibit a spurious trend in the proxy-based reconstruction. To see how this can happen, suppose that instead of proxy climate data, one simply used a random sample of autocorrelated time series that did not contain a coherent signal. If

¹University Corporation for Atmospheric Research, financial arm of the National Center for Atmospheric Research (NCAR).

these simulated proxies are standardized as anomalies with respect to a calibration period and used to form principal components, the first component tends to exhibit a trend, even though the proxies themselves have no common trend. Essentially, the first component tends to capture those proxies that, by chance, show different values between the calibration period and the remainder of the data.”

[Figure 1, Slide 2]

The NRC panel illustrated this with their own spurious hockey stick in Figure 9-2 on page 87. Our explanation of this phenomenon is similar. “... the authors make a seemingly innocuous and somewhat obscure calibration assumption. Because the instrumental temperature records are only available for a limited window, they use instrumental temperature data from 1902-1995 to calibrate the proxy data set. This would seem reasonable except for the fact that temperatures were rising during this period. So that centering on this period has the effect of making the mean value for any proxy series exhibiting the same increasing trend to be decentered low. Because the proxy series exhibiting the rising trend are decentered, the calculated variance will be larger than their normal variance when calculated based on centered data, and hence they will tend to be selected preferentially as the first principal component. ... The centering of the proxy series is a critical factor in using principal components methodology.”

[Figure 2, Slide 3]

The effect of decentering was illustrated by us in Figure 2, which is Figure 4.3 in our report. The top panel represents the North American Tree Ring PC1 as calculated based on the MBH98 methodology. The bottom panel illustrates the PC1 based on the same set of tree ring proxies with the centered PCA computation.

[Figure 3, Slide 4]

To illustrate that this spurious decentering effect is not limited to just hockey sticks we created an additional illustration based on the IPCC 1990 temperature curve. With 69 uncorrelated white noise proxies and one IPCC 1990 curve, it is clear that decentering can overwhelm the remaining proxies and preferentially select the one anomalous one.

We believe that our discussion together with the discussion from the NRC report should take the “centering” issue off the table. The decentered methodology is simply incorrect mathematics as was illustrated in our Appendix A as well as with ample simulation evidence in both our report and that of the NRC report. I am baffled by the claim that the incorrect method doesn’t matter because the answer is correct anyway. Method Wrong + Answer Correct = Bad Science. But with the centering issue off the table, the question then shifts from principal component analysis to which proxies exhibit the hockey stick shape and whether these

proxies contain valid temperature signals. We agree with Dr. Mann that the hockey stick shape is in some proxies.

[Figure 4, Slide 5]

Figure 4 is an image that I showed in our previous testimony showing just six sample Bristlecone pine proxies used in the construction of the North American PC1 series. The hockey stick shapes are clearly visible in the last two proxies. Given our discussion, it is clear how the decentering methodology will select these and give them prominence in PC1. Are these valid temperature proxies? I quote from our report, “Graybill and Idso (1993) specifically sought to show that Bristlecone Pines were CO₂ fertilized. Bondi et al. (1999) suggest [Bristlecones] ‘are not a reliable temperature proxy for the last 150 years as it shows an increasing trend in about 1850 that has been attributed to atmospheric CO₂ fertilization.’ ... We also note that IPCC 1996 report stated that ‘the possible confounding effects of carbon dioxide fertilization need to be taken into account when calibrating tree ring data against climate variations.’” At the very least, the effect of these proxies on temperature reconstruction should be examined.

[Figure 5, Slide 6]

Figure 5 shows Dr. Mann’s own illustration (MBH, Internet, 2003) of the direct effect of North American tree ring data on reconstruction results in the 15th

century. Indeed, it is our understanding as outsiders that all parties agree as to the significance of this tree ring network to final results. And that has made the use of the tree ring network a disputed issue.

[Figure 6, Slide 7]

Figure 6 is also a repeat graphic from my previous testimony. Please note that the Bristlecone/Foxtail PC1 proxy is used not only in MBH, but also in virtually every subsequent reconstruction. We do not claim to be experts in dendrology, but it seems to us as outsiders that there are sufficient confounding factors that proxies based on Bristlecones should be avoided. We should add that we were specifically asked to resolve the differences between MBH98/99 and MM03/05a/05b. There is a bewildering array of subsequent work that we were not asked to consider, but which probably deserves much more intense scrutiny. We would include such refereed papers as Rutherford et al. (2005) and Wahl and Ammann (2006), which are purported to be written by independent teams, but which are co-authored by Dr. Mann himself in Rutherford et al. and by Dr. Mann's student Dr. Ammann in Wahl and Ammann.

[Figure 7, Slide 8]

Indeed, far from there being uniform agreement on the hockey stick shape, Bürger and Cubasch (2005) have reported that a discomfoting array of different

results can be obtained from MBH proxies under minor methodological differences. Figure 7 illustrates that while there may be reasonable consensus on warming since 1900, i.e. the calibration period, as the NRC report suggests, paleoclimate temperature reconstruction past 1600 is much more problematic. Indeed, on the matter of consensus, the NOAA website titled *A Paleo Perspective ... on Global Warming* has the following contradictory statements: “The latest peer-reviewed paleoclimatic studies appear to confirm that the global warmth of the 20th century **may not necessarily** be the warmest time in Earth's history, what is unique is that the warmth is global and cannot be explained by natural forcing mechanisms.”

From <http://www.ncdc.noaa.gov/paleo/globalwarming/paleobefore.html>

Also from the same website: “In summary, it appears that the 20th century, and in particular the late 20th century, is likely the warmest the Earth has seen in at least 1200 years.”

From <http://www.ncdc.noaa.gov/paleo/globalwarming/medieval.html>

[Figure 8, Slide 9]

We do agree with Dr. Mann on one key point: that MBH98/99 were not the only evidence of global warming. As we said in our report, “In a real sense the paleoclimate results of MBH98/99 are essentially irrelevant to the consensus on climate change. The instrumented temperature record since 1850 clearly

indicates an increase in temperature.” We certainly agree that modern global warming is real. We have never disputed this point. We think it is time to put the ‘hockey stick’ controversy behind us and move on.

I would like to make it clear that our role as statisticians in the hockey stick game is not as players in the hockey game, but as referees. What we have seen and continue to see is that, notwithstanding the efforts by Dr. Nychka and others at NCAR, there is relatively little interaction between the statistical community and the climate science/meteorology communities although the latter frequently use statistical techniques. Statisticians in general have to pay their mortgages just like everyone else and in general cannot afford to do pro bono work such as we have been doing. We advocated in our report that if statistical methods are being used, then statisticians ought to be funded partners engaged in the research to insure as best we possibly can that the best quality science is being done. Drs. Nychka and Bloomfield, the statisticians involved with the NRC report, raise other issues on calibration, validation, and full quantification of uncertainty in these studies. Indeed there are a host of fundamental statistical questions that beg answers in understanding climate dynamics.

Sampling

How were the 70 trees in NOAMER 1400 selected?

4 Arkansas
4 Arizona
13 California
12 Colorado

3 Georgia
1 Louisiana
1 Montana
1 North Carolina
5 New Mexico
14 Nevada
3 Oregon
1 South Dakota
3 Utah
1 Virginia
4 Wyoming

How representative are these trees of the population of trees that grew from 1400-2000? In terms of geography, altitude, and type. If these trees seemed “interesting” to various individuals who took the core samples, do you believe those trees can/should be treated as a “random sample”? Are there biases in the selection of these trees? Presumably many trees could not be sampled because they had died or been harvested. What is the effect of this “censoring” on your data (and your analysis)? Similar questions exist about ice cores and how representative such data might be. What are the effects of gas diffusion in the ice core layers?

Analysis

What is the correlation between temperature and tree ring growth? What calibration studies have been performed? The rescaling steps taken seem to suggest that the correlation must be near 100%. Is that the case? The temperature proxy search is a regression problem. Why did you choose to use principal components (not appropriate for finding a nonstationary mean)? What weights do you use to combine different proxy types? Why? If the data are not a

random sample, then what confidence can be given to any modeling and to any “error bars”?

Forecasting and Modeling

CO₂ modeling shows a rapid increase in the near term. What do the models show in the longer term? Given the apparent high correlation between CO₂ and temperature in the model outputs, how direct is the link in the model itself? What is the difference between a true forecast and a “model run”? Do you believe your model runs have any statistical validity? The output looks like a Taylor series with no higher order terms?

Planning Experiments

What data should be collected that would be most cost-effective in increasing our understanding of the climatic models and the underlying physics (and statistics)? Is all data valuable? How does one avoid the desire to collect data at sites that appear “interesting” beforehand? What are the parallels between modern experimental science and experimental medical research of the 1960's? How many surgeons were “certain” their treatments were superior or that drugs were safe and found out otherwise with carefully designed and controlled studies? Is the risk of global warming so acute that such studies are deemed unwise?

Our report is not aimed at criticizing Dr. Mann or his colleagues, but in outlining a path for doing the science better. We note that the American Meteorological

Society has a Committee on Probability and Statistics. I believe it is amazing for a committee whose focus is on statistics and probability that of the nine members only two are also members of the American Statistical Association, the premier statistical association in the United States, and one of those is a recent Ph.D. with an assistant professor appointment in a medical school. The American Meteorological Association recently held the 18th Conference on Probability and Statistics in the Atmospheric Sciences (January, 2006). Of the 62 presenters at a conference with a focus on statistics and probability, only 8 (12.9%) are members of the American Statistical Association. I believe these two communities should be more engaged and if nothing else our report should highlight to both communities a need for additional cross-disciplinary ties.

Testimony of Edward J. Wegman

House Committee on Energy and Commerce

July 27, 2006

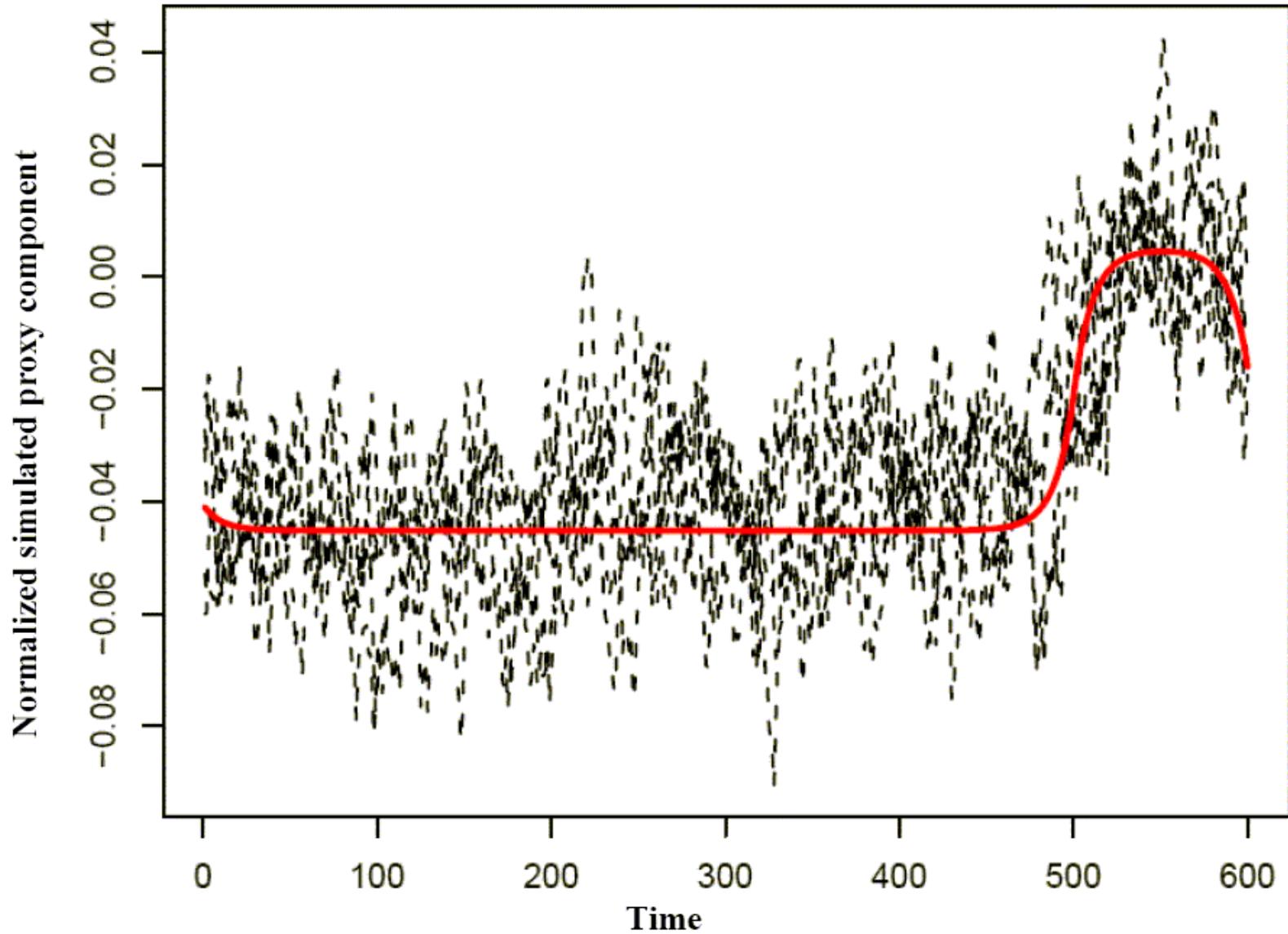
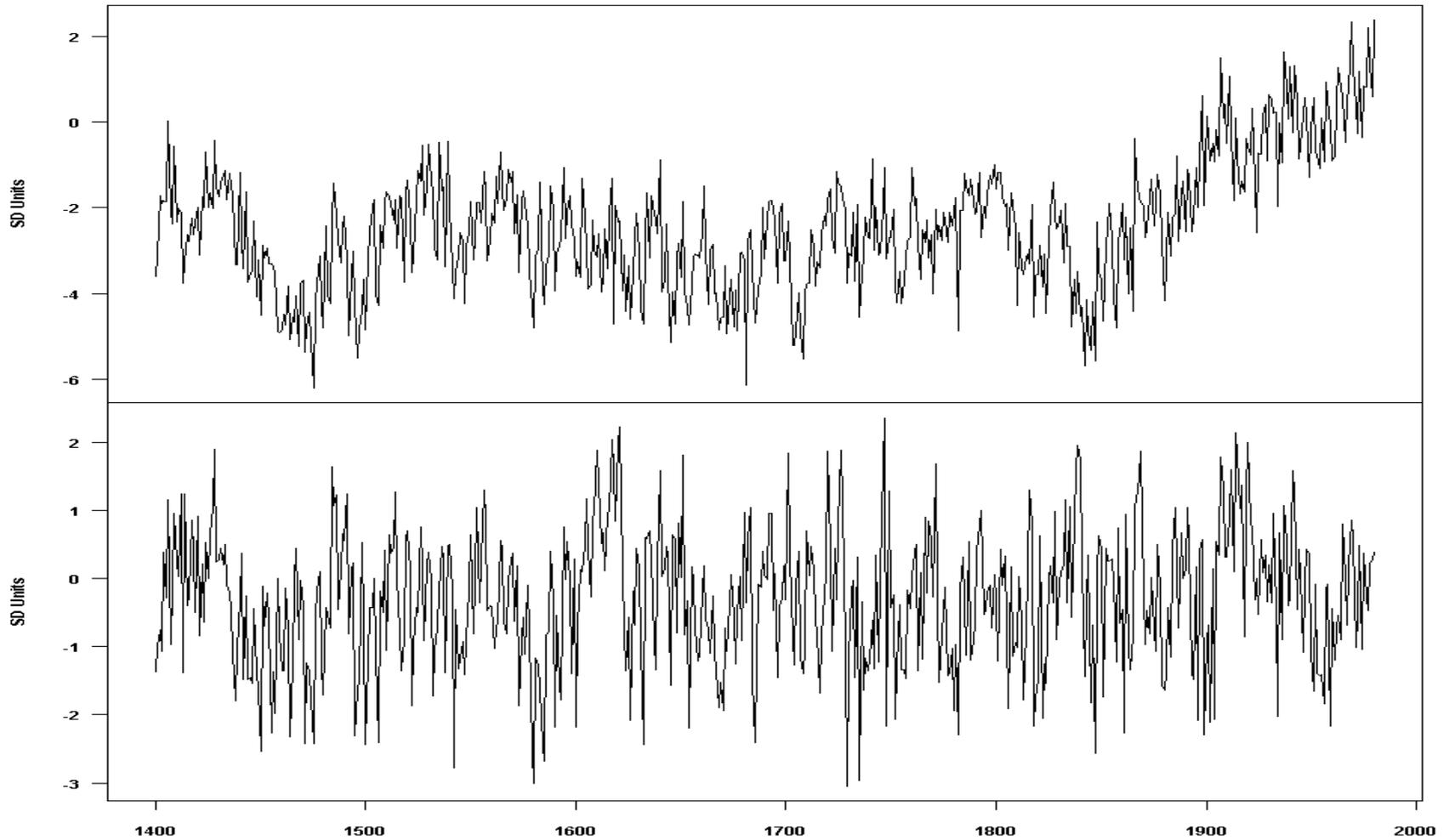


Figure 1 – Spurious Hockey Stick as reported in the NAS Panel report (Figure 9-2 in the NAS report).

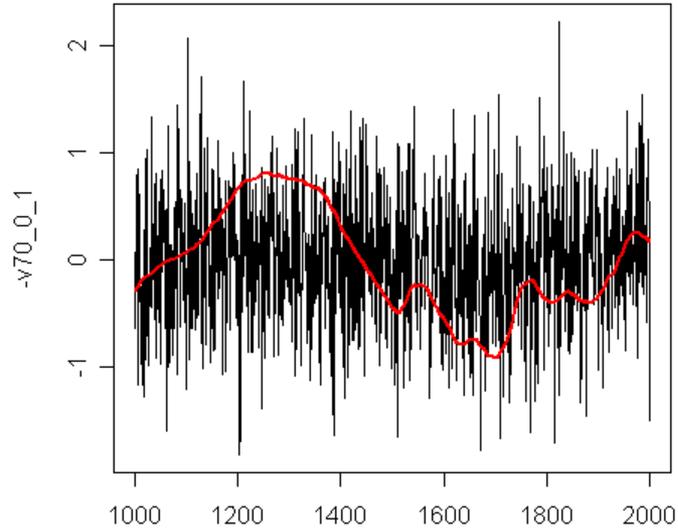


The North American Tree Network PC1 proxy was featured in MBH98. The top panel is the PC1 using the MBH98 methodology. The bottom panel is the centered PCA reconstruction.

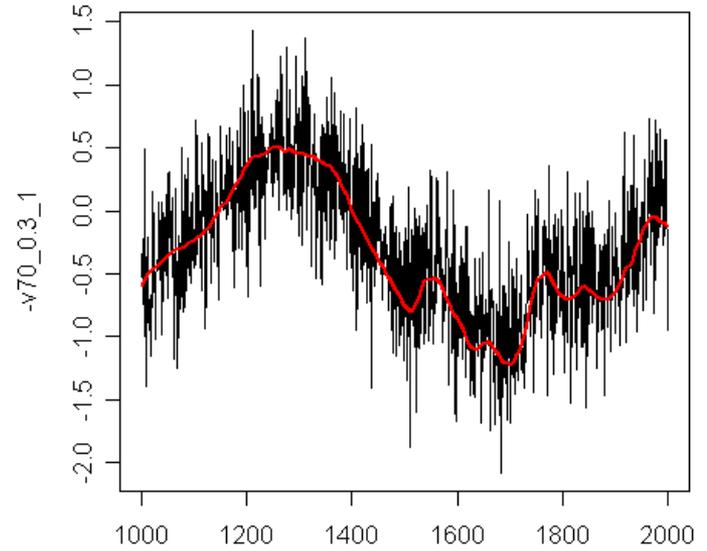
Figure 2

CFR Methods

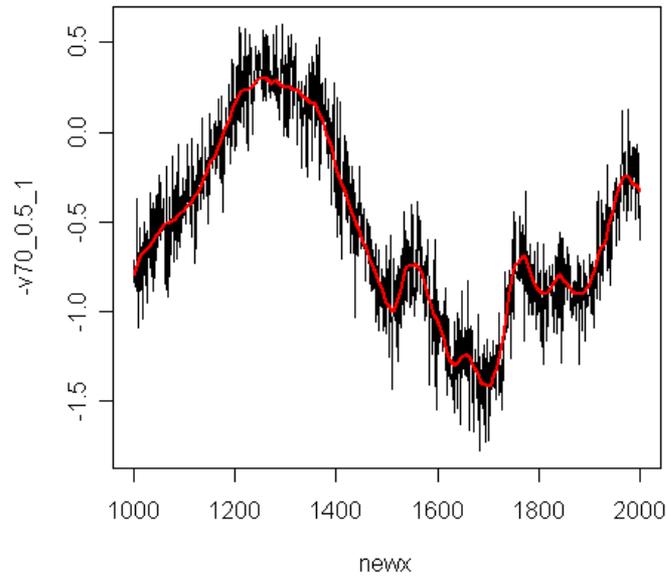
PC1 with data - 0.0



PC1 with data - 0.3



PC1 with data - 0.5



PC1 with data - 1

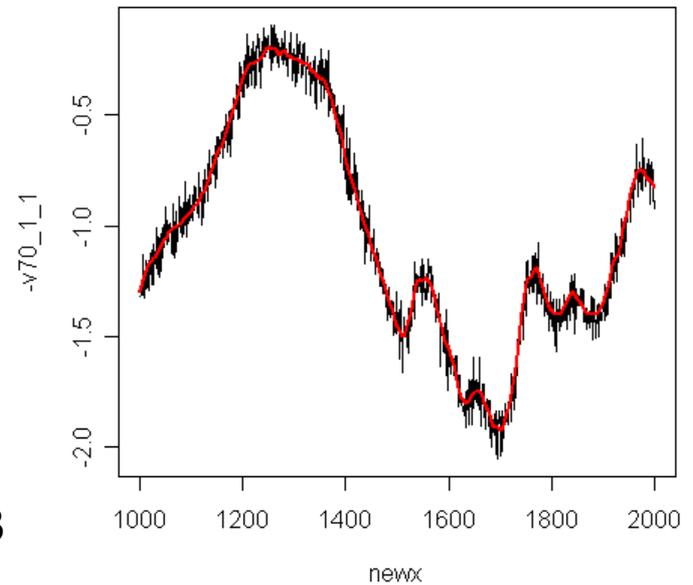


Figure 3

Sample Proxy Series

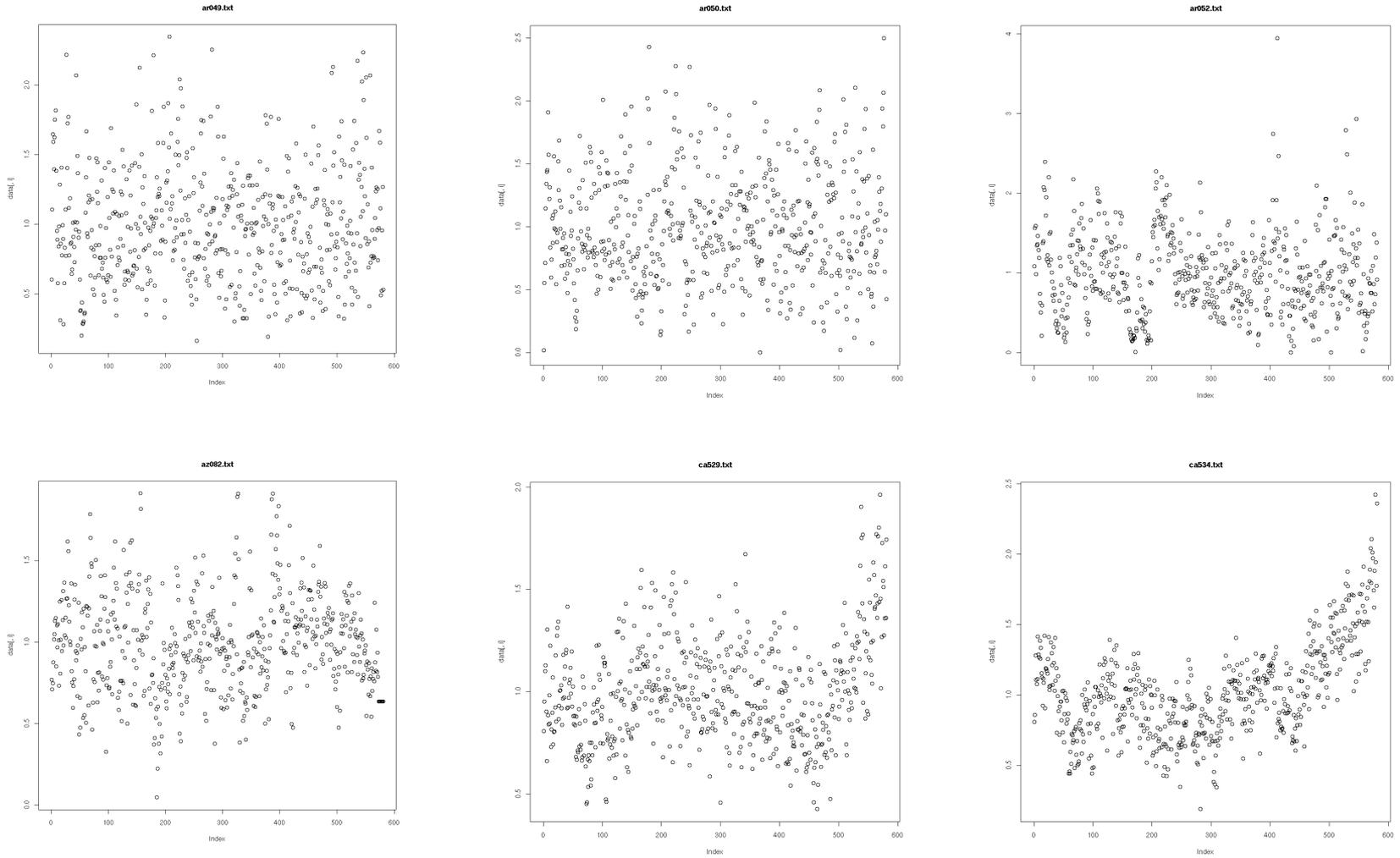


FIGURE 4

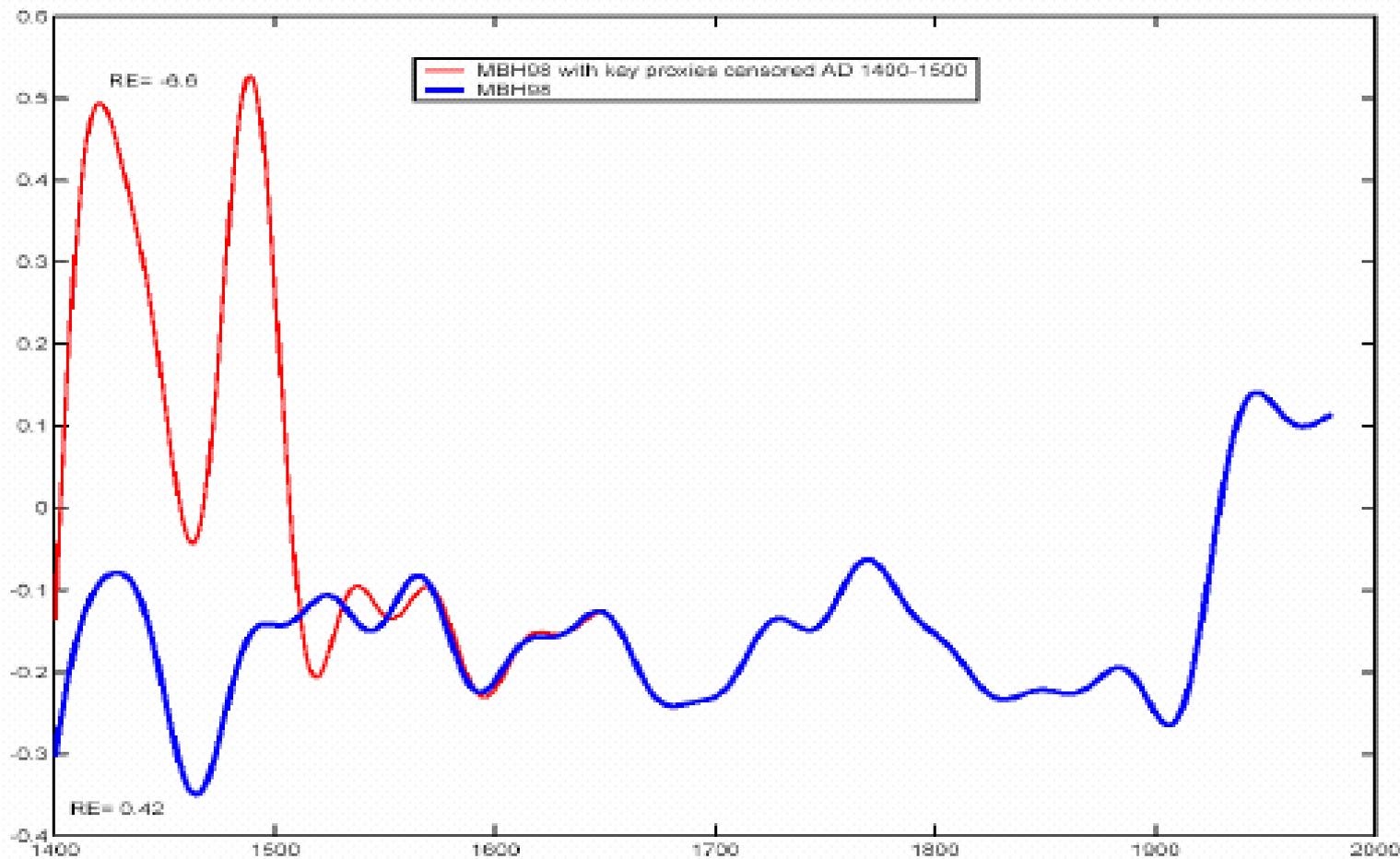


FIGURE 1. COMPARISON OF MBH98 RECONSTRUCTION (BLUE) WITH RECONSTRUCTION RESULTING FROM THE ELIMINATION OF KEY PROXY DATA SETS (1)-(3) OVER THE AD 1400-1500 INTERVAL. THIS YIELDS ESSENTIALLY THE SAME RESULT OBTAINED BY MM BY THE APPARENT ELIMINATION OF THESE DATASETS OVER THE 1400-1600 PERIOD. (BOTH SERIES HAVE BEEN SMOOTHED WITH A 40 YEAR LOWPASS FILTER).

Figure 5



Bradley & Jones (1993)

Mann, Bradley, Hughes
(1998,1999)

Jones et al. (1998)

Crowley & Lowery (2000)

Briffa (2000)

Esper (2002)

Mann & Jones (2003)

Bradley, Hughes, Diaz
(2003)

Jones & Mann (2004)

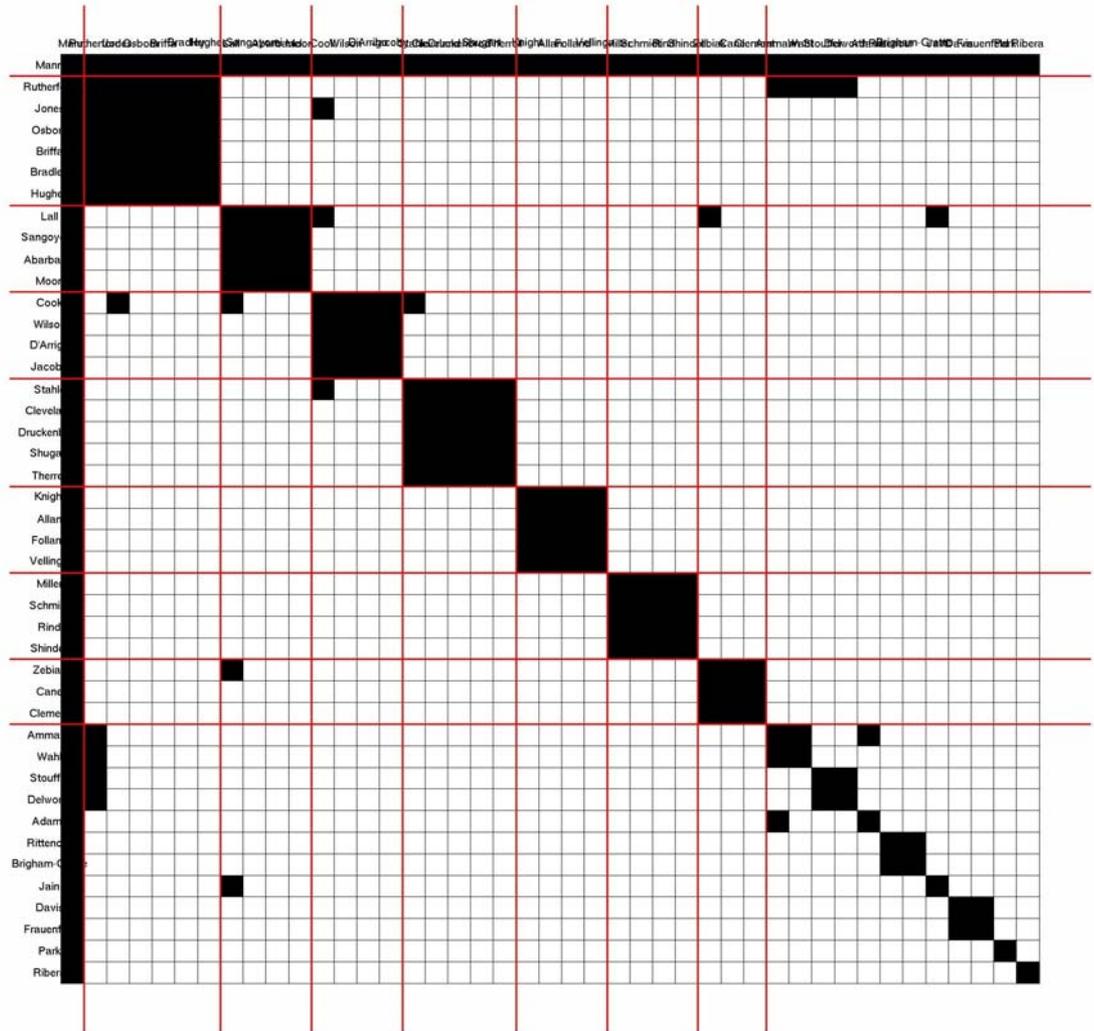
Moberg et al. (2005)

Osborn & Briffa (2006)

D'Arrigo, Wilson, Jacoby
(2006)

FIGURE 6

Rutherford, S., Mann, M. E., Osborn, T. J., Bradley, R. S., Briffa, K. R., Hughes, M. K., and Jones, P. D. (2005) “Proxy-based Northern Hemisphere surface reconstructions: Sensitivity to method, predictor network, target season, and target domain,” *Journal of Climate*, 18, 2308-2329.



Mann-Rutherford-Jones-Osborn-Briffa-Bradley-Hughes

BACKUP FIGURE 3

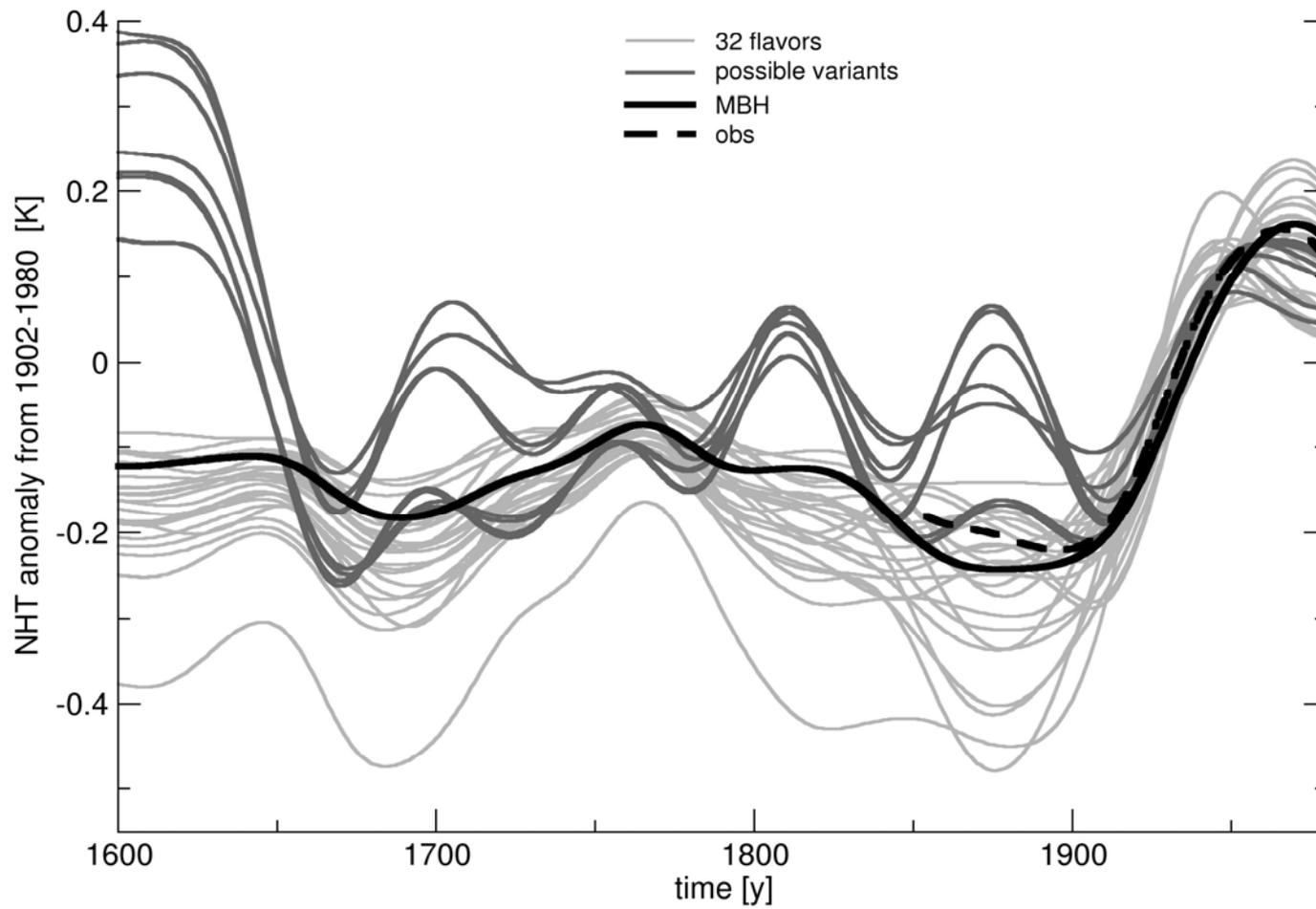


Figure 7

GLOBAL TEMPERATURE ANOMALIES

SOURCES: NOAA, NASA/GISS, AND CRU

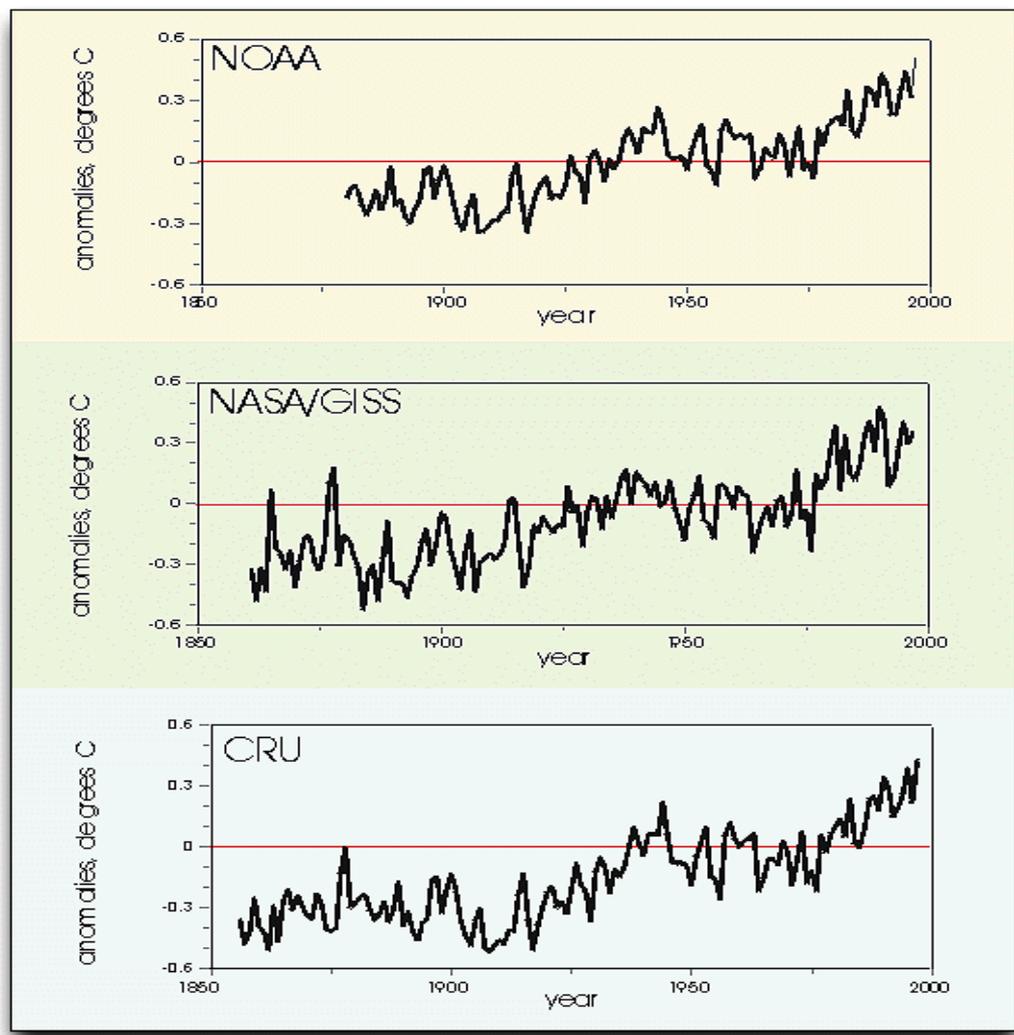


Figure 8

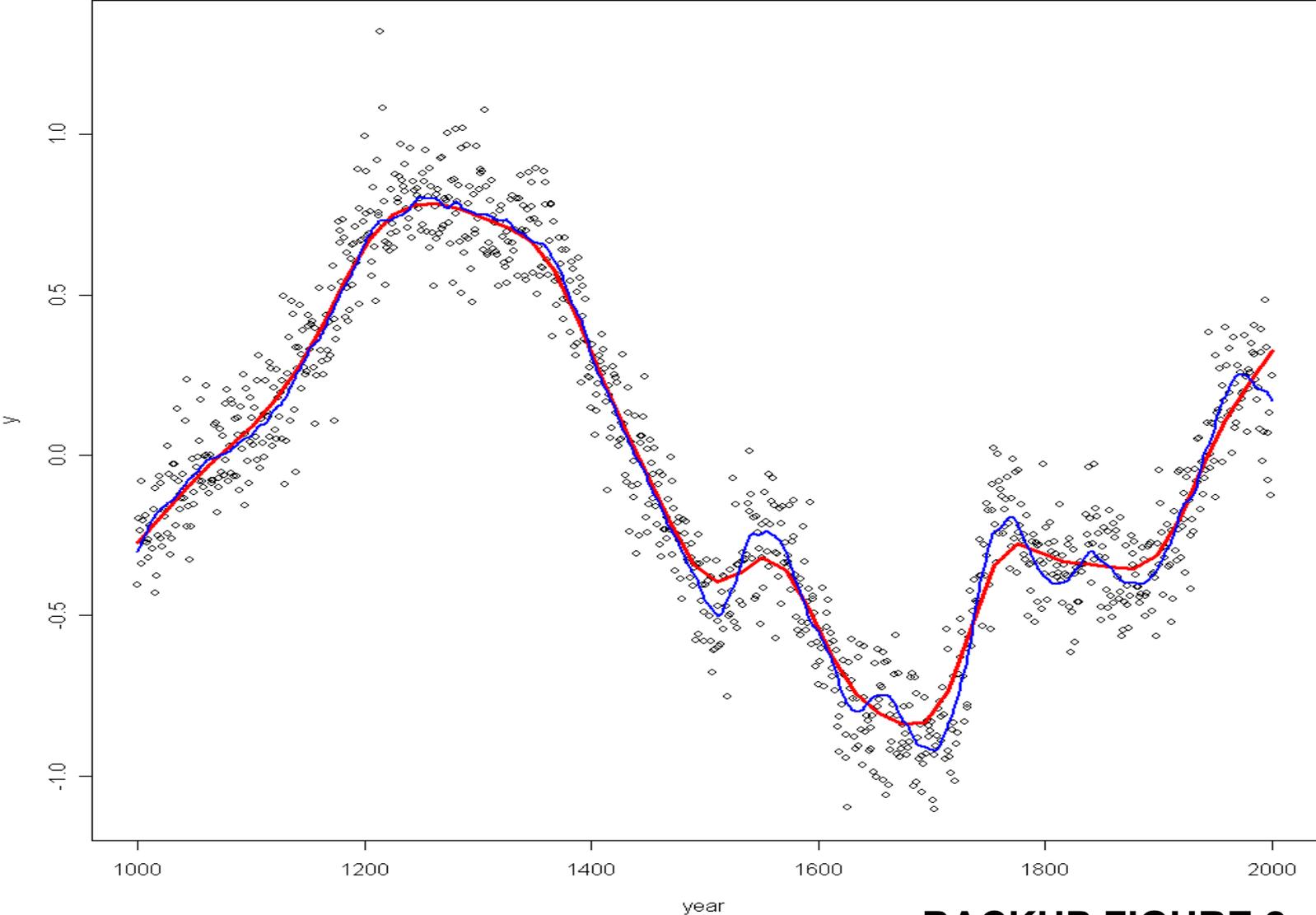
Digitized IPCC 1990 Temperature Curve



BACKUP FIGURE 1

CPS Method

Average of 70 samples of White noise and Temp signal



BACKUP FIGURE 2